

Interactive comment on “High-resolution ice nucleation spectra of sea-ice bacteria: implications for cloud formation and life in frozen environments” by K. Junge and B. D. Swanson

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In this manuscript, Junge and Swanson evaluate the potential that bacteria and viruses present in the Arctic contribute to the formation of polar clouds and sea ice due to ice nucleation activity. This question is motivated by the proposition of Bigg and Leck (2001) that such organisms participate in ice and cloud formation. However, the authors suggest that this is conjecture given that the ice nucleating capacity of most polar marine psychrophiles has not been examined. Therefore, in this manuscript the authors present data on the freezing profiles of strains of 12 different species (or groups) of marine psychrophilic bacteria and a virus (bacteriophage).

General remarks

The methods used in this work are clearly explained and the experiments are well conducted with appropriate use of controls such that the data can be readily interpreted. On the other hand, the context of their observations needs further clarification in order to justify the conclusions made by these authors. The points that need clarification are as follows:

1) How could ice nucleation active bacteria be involved in the formation of polar clouds? Do these clouds consist primarily of ice? If so, does ice form directly from the vapor phase? And if this is the case, then how are their measurements of ice nucleation pertinent to this? If polar clouds consist of liquid water droplets, then what would be the role of ice nucleation active bacteria in their formation? Is it assumed that ice nucleation active bacteria would also function as cloud condensation nuclei? The processes involved that would implicate ice nucleation relative to the formation of polar clouds should be specified a bit more.

2) Why did the authors choose the strains used in this study? Do these bacteria and the bacteriophage represent the most dominant organisms found in Arctic milieu? This issue is critical in their counter-argument of Bigg and Leck's proposition. What do the results of these present authors represent with regard to the ensemble of marine psychrophiles/Antarctic micro-organisms in general? Have they missed any of the important taxonomic groups that might in fact be ice nucleation active?

3) What is the freezing depression point of sea water? If marine psychrophiles were as active as *Pseudomonas syringae* and could catalyze freezing of water at -2° to -6°C for example, would sea water be able to freeze at this temperature? In other words, would it be ecologically pertinent for marine psychrophiles to be ice nucleation active at relatively warm temperatures? Likewise, what is the warmest freezing point of artificial sea water? Is it possible to detect ice nucleation activity at relatively warm temperatures in artificial sea water?

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4) The lack of ice nucleation induction in the strains tested is remarkable. This study is perhaps one of the first studies of bacterial ice nucleation in which ultra-pure conditions were used permitting avoidance of freezing induced by 'pollutants' in the laboratory environment. This is a very nice contribution of this work - but it also raises a weakness concerning their conclusions. Perhaps this degree of freezing avoidance is typical of bacteria that are not ice nucleation active like *Pseudomonas syringae*. Such data are probably not available in the literature. If they are, the authors should mention them. If this information is not available, then it is difficult for the authors to conclude that this is a form of adaptation to extreme cold environments. The interest of their results would be greatly improved if they could add on freezing profile data for common mesophilic bacteria that are known to not produce ice nucleation proteins such as *Bacillus* spp., *Agrobacterium* spp, *Escherichia coli* or other.

Specific comments

p. 4265, l. 22: "The enclosure and freezing tube" WERE then purged ..

p. 4266, l. 8: The authors refer to "previous work" and cite a paper that is under review (Swanson, 2007). The appropriate nuances concerning chronology should be rectified before publication.

p. 4268, l. 7. The authors cite the work of Jayaweera and Flanagan (1982) as an illustration of the presence of *Pseudomonas syringae* in the Arctic atmosphere. I would like to see a copy of the original publication before accepting that this is a valid citation. There have been other reports of *P. syringae* from the poles, from Antarctica in particular. When I finally received the so-called strain from Antarctica it was not *P. syringae*. I have never been able to access the original paper of Jayaweera and Flanagan. As much as I would love to believe that *P. syringae* makes it all the way to the poles, I would really like to know the details of how the bacteria were identified in Jayaweera and Flanagan's study. This is a very critical point. If the authors could provide me with a copy of this paper, I would be very grateful.

p. 4268, l. 28. What does passivated mean? Is this a generally accepted term? (If so, sorry for my ignorance.)

p. 4270, l. 20: The correct spelling of the bacterial species is Pseudomonas FLUORESCENS.

p. 4271, l. 10: 240 what?? Are the units missing here?

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